

Received May 17, 1770.

XXXII. *Experiments on the Blood, with
some Remarks on its Morbid Appearances;*
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Read June 14 & 21.
1770.

AS the following Experiments are made on a subject generally thought important, and as the inferences which I have ventured to draw from them seem to explain some appearances in diseases, they will not, I flatter myself, be thought altogether unworthy the attention of this learned Society.

When fresh blood is received into a basin, and suffered to rest, in a few minutes it jellies, or coagulates, and soon after separates into two parts, distinguished by the names of *crassamentum* and *serum*. These two parts differ in their proportions in different constitutions: in a strong person, the *crassamentum* is in greater proportion to the *serum* than in a weak one; and the same difference is found to take place in diseases; thence is deduced, the general conclusion, that the less the quantity of *serum* is in proportion to the *crassamentum*, bleeding, diluting liquors, and a low diet, are the more necessary: whilst in some dropfies and other diseases where the
serum

serum is in a great, and the *crassamentum* in a small proportion, bleeding and diluting would be highly improper. As it is therefore supposed useful to attend to the proportions of these parts in many disorders, and even to take indications of cure from them, it has been an object with those who have made experiments on the blood, to determine the circumstances on which its more perfect separation into these two parts depends; it being obvious, that till this be done, our inferences from their proportions will be liable to considerable fallacies. Two of the latest writers on this subject agree, that if the blood, after being taken from a vein, be set in a cold place, it will not easily separate, and that a moderate warmth is necessary: this is a fact that is evinced by daily experience. They likewise say, that the heat should be less than that of the animal, or than 98° of Fahrenheit's thermometer; and that if fresh blood be received into a cup, and that cup put into water heated to 98° , it will not separate; nay, they even say, that it will not coagulate; but this, I am persuaded from experiments, is ill-founded.

EXPERIMENT I.

A tin-vessel containing water, was placed upon a lamp which kept the water in a heat that varied between 100 and 105 degrees. In this water was placed a phial, containing blood that instant taken from the arm of a person in health; the phial was previously warmed, then filled, and corked to exclude air. In the same water was placed a tea-cup half full of blood, just taken from the same person;

a third portion of the blood was then received from the same vein into a basin, and was set upon a table, the heat of the atmosphere being at 67° . Now, according to their opinion, the two former should neither have coagulated nor separated, when that in the basin began to separate; but, on the contrary, they were all three found to coagulate nearly in the same time; and those in the warm water, not only did separate as well as the other, but even sooner.

EXPERIMENT II.

The same experiment was repeated on the blood of a person that laboured under the acute rheumatism, whilst the heat of the atmosphere was no higher than 55° , and that of the warm-water was 108° ; and the result of this experiment was not only a confirmation of what was observed in the first, but it even shewed, that, that degree of heat was so far from lessening, that it increased the disposition to coagulate; for the blood in the cup and in the phial was not only coagulated, but the separation was much advanced before the whole of the blood in the basin was coagulated. Thence I am led to conclude, that the separation of the blood in a given time, is in proportion as the heat in which it stands is nearer to the animal heat, or 98° ; or greater in that heat than in any of a less degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal whilst at rest was found both to coagulate and to separate.

It is well known, that the *crassamentum* consists of two parts, of which one gives it solidity, and is by
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some called the fibrous part of the blood, or the gluten, but by others with more propriety termed the *coagulable lymph*; and of another, which gives the red colour to the blood, and is called the red globules. These two parts can be separated by washing the *crassamentum* in water, the red particles dissolving in the water, whilst the coagulable lymph remains solid. That it is the coagulable lymph which by its becoming solid gives firmness to the *crassamentum*, is proved by agitating fresh blood with a stick, so as to collect this coagulable lymph on the stick, in which case the rest of the blood remains fluid*.

The surface of the *crassamentum*, when not covered with a crust, is in general of a more florid red than the blood was when first taken from the vein, whilst its bottom is of a dark colour, or blackish. This floridness of the surface is justly attributed by some of the more accurate observers to the air, with which it is in contact; for, if the *crassamentum* be inverted, the colours are changed, at least that which is now become the upper surface assumes a more florid redness. This difference of colour, others have endeavoured to explain from the different proportions of

* It may be proper to mention here, that till of late the coagulable lymph has been confounded with the serum of the blood, which contains a substance that is likewise coagulable. But in these papers, by the *lymph*, is always meant that part of the blood which jellies, or becomes solid spontaneously when blood is received into a basin, which the coagulable matter that is dissolved in the serum does not; but agrees more with the white of an egg, in remaining fluid when exposed to the air, and coagulating when exposed to heat, or when mixed with ardent spirits, or some other chemical substances.

the red particles, or globules as they are called, which, say they, being in greater proportion at the bottom of the *Craffamentum*, makes it appear black; but, if inverted, the globules then settle from the surface which is now uppermost, and that becomes redder. But this I think is not probable; for the lymph in the *craffamentum* is so firmly coagulated, as to make it too dense, to allow of bodies even heavier than the red particles to gravitate through it; for example gold. That air has the power of changing the colour of the blood, has been long known; and the following experiment shews it very satisfactorily, and hardly leaves room to refer the appearance to another cause.

EXPERIMENT III.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places; then opening it between two of the ligatures, I let out the blood, and filled this part of the vein with air. After letting it rest a little till the air should become warm, I took off the ligature, which separated the air from the blood, and then gently mixed them, and I observed that the venous blood assumed a more florid redness, where in contact with the air-bubbles, whilst at other parts it remained of its natural colour.

There is a difference between the arterial and venous blood in colour; the former is of a florid red like the surface of the *Craffamentum*, the latter is dark or blackish like the bottom of the *craffamentum*. This change in its colour is produced on the blood as it passes through the lungs, as we see by
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opening of living animals *; and as a similar change is produced by air applied to blood out of the body, it is presumed that the air in the lungs is the immediate cause of this change; but how it effects it, is not yet determined.

As the blood is changed to a more florid red in passing through the lungs, or from the venous to the arterial system, so it loses that colour again in passing from the arteries to the veins in the extreme parts, especially when the person is in health; but every now and then we observe the blood in the veins more florid than is usual, and it likewise frequently happens in blood-letting, that the blood which comes first out is blackish, but afterwards it becomes more florid: in these cases, the arterial blood passes into the veins without undergoing that change which is natural to it.

Some of the neutral salts have a similar effect on the colour of the blood to what air has, particularly nitre; thence some have attributed the difference of colour in the arterial and venous blood to nitre, which they supposed was absorbed from the air whilst in the lungs. But we know that this is a mere supposition, for air contains no nitre. Indeed nitre is far from

* That this change is really produced in the lungs, I am persuaded from experiments, in which I have distinctly seen the blood of a more florid red in the left auricle, than it was in the right. But some authors of the greatest authority say, that they could not observe any such difference in a great number of experiments which they made; but this I should attribute to their having been later in opening the left auricle after the collapsing of the lungs than I was; for it seems probable, that whatever is the alteration produced on the blood in its circulation through this organ, after it is collapsed, this change cannot take place.

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being the only neutral salt which has this effect on the blood, almost all the neutral salts have the same. In making some experiments on this subject, I have observed a much more remarkable effect which neutral salts have upon the blood; and that is, being mixed with it when just received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates: thus, if six ounces of human blood be received from a vein upon half an ounce of Glauber's salt reduced to a powder, and the mixture agitated so as to make the salt be dissolved, that blood will not coagulate on being exposed to the air, as it would have done without the salt; and if to this mixture about twice its quantity of water be added, in a few minutes the whole will be jellied or coagulated, and on shaking the jelly, the *coagulum* will be broken, and the part so coagulated can be now separated as it falls to the bottom, and proves to be the lymph.

In these mixtures of the blood with neutral salts, the red particles readily subside (especially if human blood be used) and the surface of the mixture becomes clear and colourless; and being poured off from the red part, it is found to contain the coagulable lymph, which can be separated by the addition of water.

I have tried all the neutral salts, and have made a table of their effects on the blood, but this table I shall not trouble the Society with at present: it may be sufficient to observe that in general they agree in producing this change*. And it is less necessary to

* It may be necessary to observe here, that those made with the volatile alkali, and with the earth of allum, are to be excepted.

be particular in giving a detail of their effects, from our not knowing of any use this would be in medicine, because we must not conclude that their effects in the body would be the same as we see they are out the body. Indeed, these experiments, as well as many others, were not made so much with a view of any immediate application to medicine, as to determine the properties of the blood chemically : for, having set out with a persuasion, that a more particular acquaintance with the properties of this fluid was necessary before we could arrive at the knowledge of some of the animal functions, such as the manner in which the bile and other secreted fluids are formed, I therefore determined to do my utmost to throw some light on this subject. It was with this view that I have made some experiments even on living animals, being convinced that such experiments could not otherwise be made satisfactorily.

When blood is thus kept fluid by neutral salts, it still retains its property of being coagulable by heat, and by other substances as before, air excepted. This method of keeping the blood fluid may therefore be useful, by giving us an opportunity of making some experiments on the blood, which we could not otherwise do, from its coagulating so soon when taken from the blood-vessels.

This property of one of the neutral salts has been long known, amongst those who prepare blood for food ; for it has long been a practice with such people, to receive blood into a vessel containing common salt, and to agitate it as fast as it falls, by which means the coagulation is prevented, and the blood remains so fluid as to pass through a cloth, without

leaving any *coagulum* behind: by this means they have an opportunity of mixing it with other substances for the uses of the kitchen.

Although the coagulable lymph so readily becomes solid when exposed to the air, yet whilst it circulates it is far from being solid: it has indeed been supposed to be fibrous, even whilst moving in the blood-vessels, but erroneously.

It is this coagulable lymph which forms the inflammatory crust, or *buff* as it is called. It likewise forms *polypi* of the heart, and sometimes fills up the cavities of aneurisms, and plugs up the extremities of divided arteries. It is supposed, by its becoming solid in the body, to occasion obstructions and inflammations; and even mortifications, from the exposition to cold, have been attributed to its coagulation. In a word, this lymph is supposed to have so great a share in the cause of several diseases, that it would be desirable to ascertain what brings on that coagulation, either in the body or out of it.

The blood, when received into a basin and suffered to rest in the common heat of the atmosphere, very soon jellies or coagulates; the part which now becomes solid is the coagulable lymph, as has been shewn above. The circumstances in which it now differs from what it was in the veins, are these: it is laid open to the air, to cold, and is at rest; for whilst in the body, air is excluded, it is always of a considerable warmth, and is always in motion. The question is, to which of these circumstances its coagulation whilst in the basin is chiefly owing. This question, I believe, cannot well be answered from the experiments that have hitherto been made. It has
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indeed been said, that the cold alone coagulated it; for, say they, if you receive blood into a basin, and keep that basin in warm water, and stir the blood well, it can be kept fluid. But in the experiments from which this conclusion was made, I find there has been a deception. In short I have found that it coagulates as soon when kept warm, and when agitated, as it does when suffered to rest and to cool. As the subject seemed to me of importance, I have endeavoured to ascertain the circumstance to which this coagulation is owing, by several of experiments, in each of which the blood was generally exposed to but one of the suspected causes at a time. Thus, in order to see whether the blood's coagulation out of the body was owing to its being at rest, I made the following experiment.

EXPERIMENT IV.

Having laid bare the jugular vein of a living dog, I made a ligature upon it in two places, so that the blood was at rest between the ligatures; then covering the vein with the skin, to prevent its cooling, I left it in this situation. From several experiments made in this way, I found in general, that after being at rest for ten minutes, the blood continued fluid; nay, after being at rest for three hours and a quarter, above two thirds of it was still fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the same animal, was completely jellied in about seven minutes. The coagulation therefore of the blood in the basin, and of that which is merely at rest, are so different, that rest alone can-

not be supposed to be the cause of the blood's coagulation out of the body.

To see the effects of cold on the blood, I made this experiment.

EXPERIMENT V.

I killed a rabbit, , and cut out one of its jugular veins immediately, proper ligatures being previously made upon it; I then threw the vein into a solution of sal ammoniac and snow, in which the mercury stood at the 14th degree of Fahrenheit's thermometer. As soon as the blood was frozen I took the vein out again, and put it into luke-warm water till it thawed and became soft; I then opened the vein, received the blood into a tea-cup, and observed that it was perfectly fluid, and in a few minutes it jellied or coagulated as blood usually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not solely owing to cold, any more than it is to rest.

Next, to see the effects of air upon the blood, I tried as follows.

EXPERIMENT VI.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures and emptied that part of its blood. I next blew in warm air into the empty vein, and put another ligature upon it, and letting

ting it rest till I thought the air had acquired the same degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where in contact with it, as could be seen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated: and as the blood could not in this time have been completely coagulated by rest alone, the air was probably the cause of its coagulation.

From comparing these experiments, may we not now venture to conclude, that the air is a strong coagulant of the blood, and that it is to this its coagulation when taken from the veins is chiefly owing, and not to cold or to rest?

It may not be improper to observe here, that there are none of the above related experiments I have been obliged to repeat so often as the 4th, which was made with a view to determine whether the blood would coagulate by rest. In the first trial which I made, the vein was not opened till the end of three hours and a quarter; and just before it was opened I had observed through its coats, that the upper part of the blood was transparent, owing to the separation of the lymph. On letting out this blood, it seemed to me entirely fluid; a part indeed had been lost, but the greatest part was collected in the cup, and which afterwards coagulated as blood commonly does when exposed to the air. From this experiment I imagined that the whole had been fluid; but from others made since, I am persuaded that the part which was lost had been coagulated; for, from a variety of trials, I now find that though

the whole of the blood is not congealed in this time by rest alone, yet a part of it is. But as it would be trespassing too much on the Society's time to relate every experiment I have been obliged to make for this purpose, I shall only mention the general result of the whole.

After fixing a dog down to a table and tying up his jugular veins, I have in general found, that on opening the veins, at the end of ten minutes, the blood was still entirely fluid, or without any appearance of the coagulation's beginning *. If opened at the end of fifteen minutes, at first sight it appeared quite fluid; but on a careful examination I have found sometimes one, and sometimes two or three small particles about the size of a pin's head, which are part of the blood coagulated. When opened later than this period, a larger and larger *coagulum* was observed; but so very slowly does this coagulation proceed, that in an experiment where I had the curiosity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the *coagulum* weighed only two grains; whilst the rest of the blood, which was fluid, on being suffered to congeal, and then weighed, was found to weigh eleven grains. I can advance nothing farther in this part of my subject with precision. Nor can I pretend exactly to determine the time at which all the

* I say, in general, it was fluid at the end of ten minutes; but I must likewise mention that in one dog I found two very small particles of beginning coagulation, even at this period; yet in another I could not observe any such appearance, even at the end of fifteen minutes,

blood between the ligatures is coagulated. I have indeed opened such a vein at the end of three days, when I found a thin, white *coagulum*, which was a mere film; the *serum* and red particles having disappeared. But the whole is undoubtedly *congealed* long before this period. The manner in which the blood coagulates, when at rest in the body, has appeared to me curious, and therefore I have taken the more pains to discover how it happens, especially as it may assist us in judging whether or no it coagulates in the heart, so as to form those substances called *polypi*. The abovementioned times will, I believe, be found to be those at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human subject in health jellies out of the body nearly in the same time, that is, it begins in three or four minutes, and is completed in seven or eight; I should therefore conclude that the blood coagulates nearly at the same period in the veins of the human body. But it may be necessary to add here, that from experiments which I have made, I have reason to believe that the time at which the blood coagulates, is different in different constitutions, and in different diseases. For though the blood of a person in health is completely coagulated in seven minutes after it is taken out of the veins, yet in some diseases, I have found the blood fifteen or twenty minutes, nay even an hour and an half, before it was completely jellied.

As we see in the above related experiments, that the blood coagulates in the body when suffered to rest for a little time, is it not probable that tis to this
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cause its coagulation in those true aneurysms, which are attended with a pouch, are owing *? For in such enlargements a part of the blood is without motion, which will congeal when at rest, and in contact with the sack; and thus one layer may be formed; and the sack afterwards enlarging, another portion of the blood will then be at rest; and so a second layer may be formed; and thence probably is the origin of those laminated *thrombi* met with in such sacks.

Likewise, to the blood being at rest, is probably owing its coagulation in the large arteries which are tied after amputation, or other operations; for after most of such ligatures there will be a part of the artery impervious, in which the blood can have no motion. The *coagulum* after amputation might indeed be supposed owing to air; but, considering the manner in which arteries are tied whilst the blood is flowing from them, it does not seem probable that the air has any effect on what is above the ligature.

To the Blood's being without motion in the cavity of the *uterus*, is its coagulation therein probably owing; hence the origin of those large clots which we sometimes observe to come from this cavity, and which, when they are more condensed by the ouzing out of the *serum*, and of the red particles, assume a flesh-like appearance, and have often been called *moles* or *false conceptions*.

In Experiment the 5th, we found that the blood could be frozen and thawed again, without being coagulated: this, likewise is an experiment which

* An instance of which may be seen in the Medical Obs. and Inq. vol. i. article xxvii. fig. iii.

I have repeated several times, that I might be sure of the fact. I have also varied the experiment a little, having sometimes put the vein into a phial of water, and froze the whole in a solution of sal ammoniac in snow; and sometimes I have put the vein into the solution itself; and three or four times I have put it into oil, and then froze it; but after all these trials, the result was found to be the same. The blood was always evidently fluid on being thawed, and as evidently jellied when exposed to the air.

Thus far I have related such experiments as I have made, in order to discover the causes of the coagulation of the blood, out of the body. Next, if agreeable to this learned Society, I shall lay before them some other experiments that I have made on this fluid.